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Executive Director: EUNICE THOMAS MINER

DAVID BARNARD STEINMAN

(1886-1960)

IN MEMORIAM

DAVID BARNARD STEINMAN
(1886-1960)

David Barnard Steinman, internationally eminent bridge engineer, son of Louis Kelvin and Eva (Scollard) Steinman, was born in New York, N. Y., on June 11, 1886, and died on August 21, 1960.

He received his B.S. degree *(summa cum laude)* at The City College, New York, N. Y. and his C.E., A.M., and Ph.D. degrees at Columbia University, also in New York. He was Professor of Civil Engineering at the University of Idaho, Moscow, Idaho from 1910 to 1914, and Professor of Civil and Mechanical Engineering at The City College from 1917 to 1920. Since 1920, he had been in private practice and had served as designing or consulting engineer in the construction of more than 440 bridges on five continents. Eight of his bridges have been honored in the annual awards for the most beautiful bridges in America. He had received hundreds of honors, both national and international, for distinguished achievements. He had twice received the Norman Medal, the highest award of the American Society of Civil Engineers, New York, N. Y., in addition to two other awards of the society. In 1932 he was presented with a silver scroll by 11 engineering societies for his outstanding contributions to the advancement of the engineering profession. He is the author of an inspirational biography, *The Builders of the Bridge* (1945); a number of standard works on bridge design and construction; and more than 600 technical papers and articles, many of them translated and published around the world. In 1947 he was awarded an honorary Doctor of Science degree by his alma mater and the University Medal for Excellence by Columbia University, followed by the Egleston Medal in 1951. In 1952 he was presented with the highest award of the National Society of Professional Engineers, Washington, D.C. In 1953 he had nine more honorary degrees conferred upon him (six in the United States and three in Europe), including that of Doctor of Science by Columbia University and the University of Ghent, Belgium; Doctor of Civil Engineering by the University of Bologna, Italy; Doctor of Engineering by Rensselaer Polytechnic Institute, Troy, N. Y., and Manhattan College, New York, N. Y.; and Doctor of Laws by Alfred University, Alfred, N. Y. He had received, also, 10 more honorary doctorates, bringing his total number of academic degrees to 24, including 4 earned and 20 *honoris causa*. In 1954 he received the highest award of the Scientific Research Society of America, New Haven, Conn., for his research and inventions in suspension bridge aerodyamics. He has been honored as President, Honorary Member and Fellow, of a long list of engineering and scientific societies. Also, Steinman served in the capacity of two officerships during his incumbency on the Council of The New York Academy of Sciences, namely: Councilor 1951 to 1953; President, January 1, 1953, to December 31, 1953; and

Past President during the years 1954 to 1955. He was awarded the French Legion of Honor and many other honors by foreign countries.

A few years ago Steinman completed the world's largest building project, the $100,000,000 Mackinac Bridge in Michigan. Among the many notable bridges on which he was designer or consultant are the Florianopolis Bridge in Brazil, the Mount Hope Bridge in Rhode Island, the St. Johns Bridge in Oregon, the Carquinez Straits Bridge in California, the Henry Hudson Bridge in New York, the Constitution Bridge in Puerto Rico, the Thousand Islands International Bridge, the Baghdad Bridge over the Tigris River in Iraq, the Kingston Bridge across the Hudson River, the Raritan River Bridge in New Jersey, and the Connecticut Turnpike Bridge in New Haven. He was engaged on plans for an intercontinental bridge over the Bosporus, a long-span steel arch bridge in Pakistan, and an international bridge between Sault Ste. Marie, Mich., and Sault Ste. Marie, Ontario, Ont., Canada. In 1958 he completed another international suspension bridge over the St. Lawrence Seaway. He was the founder and president of the David B. Steinman Foundation, New York, N. Y., for grants to education, for research, and for student aid. Steinman was internationally recognized for his original mathematical and scientific contributions to bridge engineering and other sciences. He was a pioneer leader of professional engineers in the nation. He headed the National Society of Professional Engineers.

Steinman achieved recognition as an engineer, scientist, mathematician, artist, inventor, bridgebuilder, educator, lecturer, author, poet, and humanitarian. His life and his work have been an inspiration to thousands of young engineers and engineering students not only in America, but throughout the world.

BORIS PREGEL

SECTION OF GEOLOGICAL SCIENCES

STRUCTURAL HISTORY OF TRIASSIC ROCKS OF THE CONNECTICUT VALLEY BELT AND ITS REGIONAL IMPLICATIONS*

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Introduction

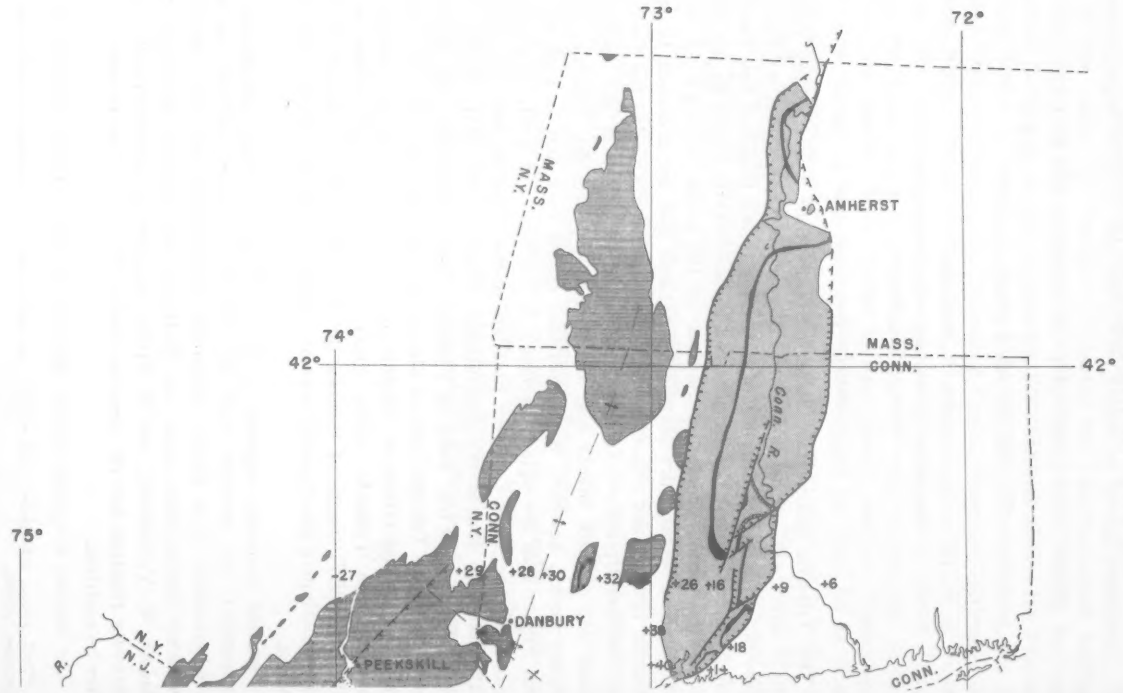
The Triassic sedimentary rocks generally form a low rolling valley 5 to 20 miles wide called the Central Lowland of Connecticut and Massachusetts, which extends northward from Long Island Sound at New Haven, Conn., across central Connecticut and Massachusetts nearly to the Massachusetts-Vermont border, a distance of 105 miles (FIGURE 1). This lowland lies between two plateaulike uplands that are underlain by pre-Triassic igneous and regionally metamorphosed rocks called the Eastern and Western Uplands respectively. The tilted edges of the resistant basalt and dolerite sheets that are intercalated with the Triassic sedimentary rocks, however, form prominent ridges that rise as high as or even higher than the upland surfaces to the east and west. Throughout most of its length, the eastern boundary of the Triassic belt is a border fault that also formed the depositional border of the basin during the Upper Triassic. From west of Hamden, Conn. northward, the western boundary of the Triassic belt is also a fault contact, but this is a later fault that did not influence Triassic sedimentation but merely served to downdrop the Triassic rocks, which formerly extended farther west (Wheeler, 1937). South of Hamden the western border is formed by the eastward-dipping pre-Triassic erosion surface at the base of the Triassic section.

Detailed geologic mapping in the Branford 7½-minute quadrangle in Connecticut has revealed that the warped structures outlined by the once-horizontal lava flows, which are interbedded with the Triassic nonmarine sedimentary rocks, are limited to the Gaillard graben (J. E. Sanders, C. V. Guidotti, and P. Wilde: unpublished observations). This discovery requires that the previous theory of origin of the warped structures be modified.

The similar structural history that can be inferred from both the Con-

*This paper, illustrated with slides, was presented at a meeting of the Section on November 7, 1960.

The Section of Chemical Sciences held a meeting on November 1, 1960, at which Albert Wassermann of University College, London, England presented a paper on "Deeply Colored, Electrically Conducting Polymers." This paper will be published in a subsequent issue of the *Transactions*.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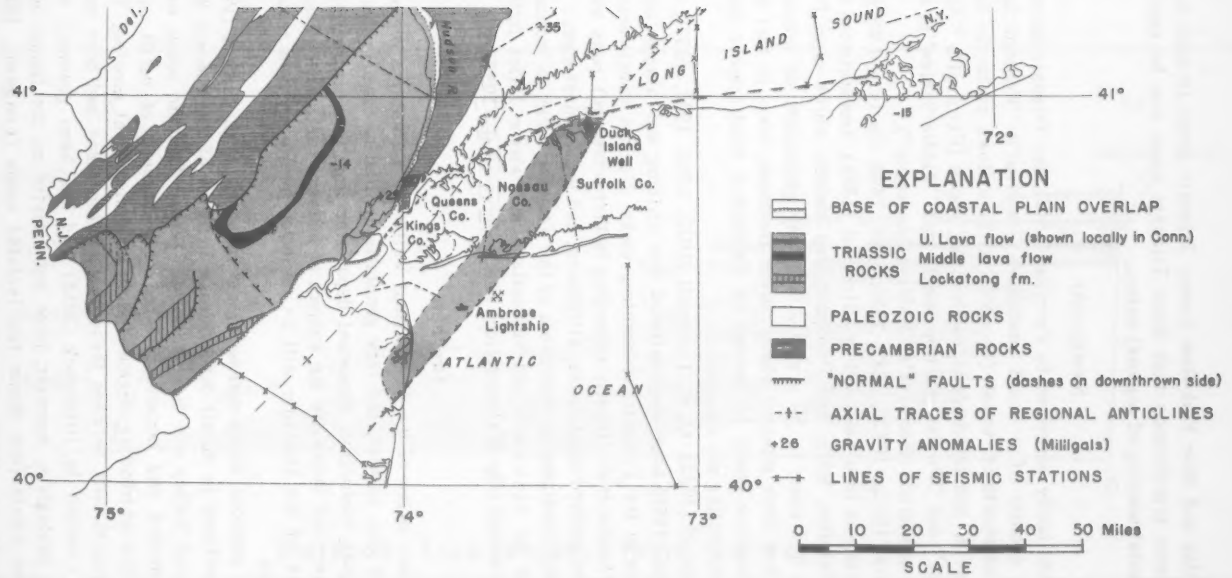

FIGURE 1. Schematic tectonic map of Triassic rocks of Connecticut Valley, New York-New Jersey belts and contiguous areas (intrusives omitted). Base from United States Geological Survey maps; geology compiled from state geologic maps of Connecticut, Massachusetts, New York, and New Jersey.

necticut Valley and New York-New Jersey Triassic areas is held to be of such genetic significance that these Triassic areas can be used to document crustal warping of regional extent.

Stratigraphy

As first brilliantly perceived by Percival (1842), the Triassic rocks of Connecticut consist of a basal sandstone (Percival's "Western Sandstone"), three sheets of basalt (accepted as lava flows since the work of W. M. Davis, and interbedded sedimentary rocks (Percival's "Anterior," "Main," and "Posterior" trap sheets and "Middle Shales"), and an overlying sandstone (Percival's "Eastern Sandstone").

W. M. Davis (1882*a*, 1882*b*, 1883, 1886, 1888, 1898; Davis and Whittle, 1889), following an idea proposed by Hitchcock (1858), demonstrated that Percival's Anterior, Main, and Posterior trap sheets, which after Percival's time had come to be regarded by many geologists as intrusive sheets that had been injected along curved fractures, were in fact lava flows and therefore could be treated as distinctive stratigraphic datum planes for interpreting the structure.

W. L. Russell (1922), C. R. Longwell (1922, 1928, 1933, 1937), and P. D. Krynine (1950) have demonstrated that striking and abrupt facies, changes occur in the Triassic sedimentary rocks. Boulder conglomerates close to the border fault (locally extending farther westward) pass westward into finer grained sandstones, siltstones, and local limestones.

The general stratigraphic framework of the Triassic rocks is indicated schematically by FIGURE 2 and summarized in TABLE 1 (for further details and a recent map of Connecticut see Rodgers *et al.*, 1959).

Structure

The Triassic rocks of the Connecticut Valley belt display a regional southeastward dip; in fact, the term generally applied to them is southeastward-dipping *homocline*. Important departures from this configuration occur, however, and these are the subject of interest here. The previous interpretations of the structure will be reviewed before my own results are considered.

Review of previous interpretations of the structure. I. C. Russell (1878, 1880) first outlined in detail the evidence in favor of the concept that the Connecticut Valley and New York-New Jersey Triassic areas were formerly connected and became isolated only because of uplift on a northeast-southwest-trending regional arch and subsequent erosion. The idea of a regional arch between these two outcrop areas had been mentioned briefly earlier by Hitchcock (1858) and has been generally accepted. Most geologists, however, have not insisted on the former connection of the strata from these two isolated areas (Longwell, 1932, 1933; and Woodworth, 1932). Barrell (cited by Longwell, 1922) named

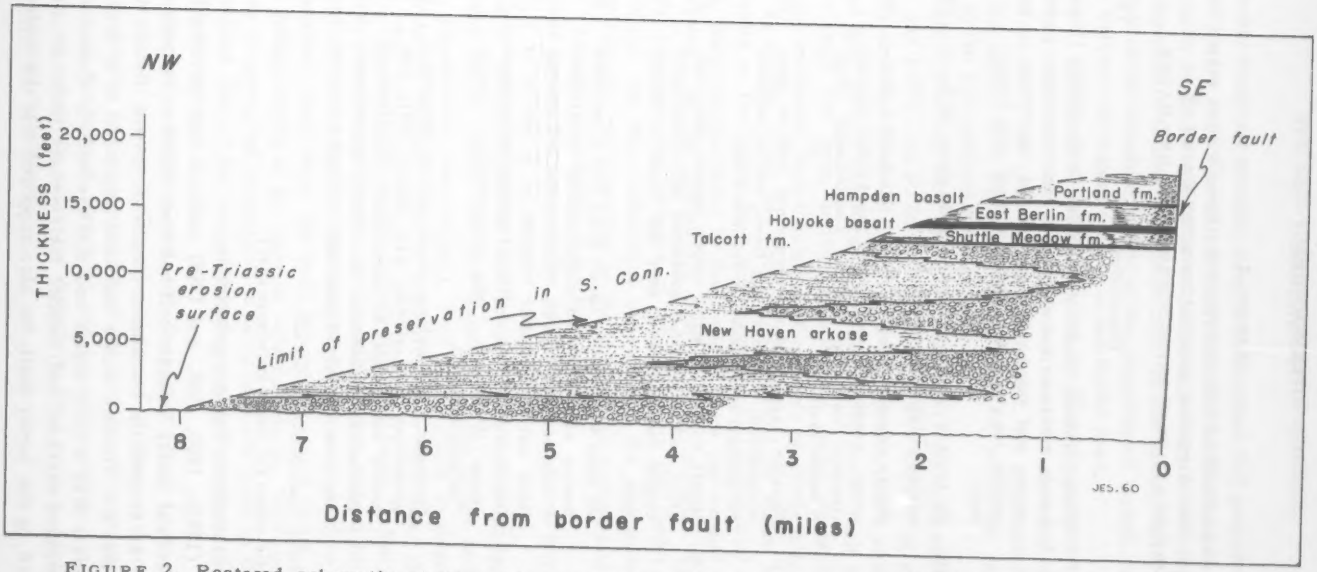

FIGURE 2. Restored schematic stratigraphic cross-section of Late Triassic rocks, latitude of New Haven, Conn. (intrusive rocks omitted).

TABLE 1

FORMATIONS OF CONNECTICUT TRIASSIC ROCKS WITH DESCRIPTIONS AND THICKNESSES BASED ON OBSERVATIONS IN AREAS SURROUNDING NEW HAVEN

Name	*Description*	*Thickness*
Portland formation	Mostly covered; scattered exposures of thin-bedded maroon siltstones and claystones, local black siltstones and interbedded gray graded sandstones; boulder conglomerates locally at base near border fault.	Estimated 1000 ft. preserved
Hampden basalt	At least 2 sheets of basalt, with about 20 ft. of maroon siltstone interbedded.	150 ft.
East Berlin formation	Mostly covered in strike valley; thin-bedded micaceous sandstone near base; thin-bedded maroon siltstone near top; boulder conglomerate close to border fault.	1500 ft.
Holyoke basalt	Massive basalt and coarser-grained rock of same composition; probably many flows, but no evidence for multiple flows known in southern Conn.	600 ft.
Shuttle Meadow formation	Mostly covered in strike valley; maroon shaly siltstone and thin-bedded sandstone with interbedded cobble conglomerates near top close to border fault.	1200 ft.
Talcott formation	At least 4 distinctive basalt flows with interbedded maroon siltstones and coarse-grained sandstones; 1 flow contains pillows, flow breccia and mineral-lined cavities; another consists of a breccia of basalt blocks with matrix of quartz and feldspar grains (Percival's "trap conglomerate"; Davis' "tuffaceous trap").	400 ft.
New Haven formation	Generally structureless, poorly sorted feldspathic sandstones and arkoses, with maroon shaly siltstones and pebble- to cobble-sized conglomerates interbedded; sandstones are locally cross-bedded and show cut-and-fill structures; outcrops scattered.	Estimated 15,000 ft.
~~ Nonconformity ~~~		
Milford chlorite schist (of Lower Paleozoic age?)		

this regional arch the Taconic geanticline.

Davis (1882*b*, 1886, 1888, and 1898) realized that the lava flows in the Connecticut Valley Triassic belt had been folded subsequent to their extrusion as essentially continuous horizontal sheets. Davis, however, thought that the Triassic strata had been deposited in a downwarped trough rather than a fault trough and that the beds in Connecticut formerly extended eastward well beyond their present limits of outcrop as determined by the border fault. He also supposed that the folding about

northwest-southeast-trending axes was an event that preceded movement on the border fault. Davis' interpretation of the offsetting of the warped structures by faults stands as a classic milestone in structural geology, but important revisions have been made in his interpretation of the mechanics of the structure, origin of the depositional trough, and relationships between the warped structures and border fault.

Barrell (1915) first suggested that the Triassic sediments had been deposited under nonmarine conditions in a trough that had subsided by flexure at first, but later by fault movement. He proposed the concept that the border fault had originated during sedimentation and also had been active after sedimentation ceased.

Barrell's idea was taken up by W. L. Russell (1922), Longwell (1922, 1932, 1933, and 1937), and P. D. Krynine (1950), who showed on petrographic grounds that the Triassic rocks could not possibly have extended much beyond their present eastward termination at the border fault and that the border fault had been active intermittently during deposition of the Triassic sediments, thus maintaining the supply of coarse debris.

W. L. Russell (1922) and Wheeler (1939) related the warped structures to movements on the border fault and suggested that differential subsidence during downward movement by the lowland block had caused the warped "anticlines" to lag behind the more actively sinking "synclines." Wheeler proposed that changes of attitude along the border fault were responsible for this differential subsidence, the "anticlines" being located opposite protuberances on the border fault surface and the "synclines" opposite re-entrants in the fault surface.

I have discussed some of the inadequacies of these interpretations (1958).

Present interpretation of structures. The present interpretation of structural history stems from the discovery of the Foxon fault (Sanders *et al.*, unpublished observations). This fault, which is downthrown on the southeast, together with the Triassic border fault forms the Gaillard graben. The restriction of the Saltonstall "half-basin," North Branford anticline, Totoket half-basin, and possibly other warped structures to this graben block suggests that warped structures might form only on a fault-bounded block where the beds between bounding faults are free to deform independently of the beds on adjacent blocks. This contention is not easy to prove, but indications in favor of it are found in the correlation between size of the warped structures and dimensions of the block on which they occur.

Problem of syndepositional and postdepositional fault movements. The question of the two different episodes of faulting, one accompanied by sedimentation and no warped structures and the other accompanied by formation of the warped structures and no sedimentation, is somewhat involved and cannot be subjected directly to field testing. Several possibilities suggest themselves:

(1) The lowland block moved relatively downward in both episodes but, in between, some tectonic change occurred so that the mechanical effects of the first episode differed from those in the second, even though downward movement occurred in both. This interpretation demands that one postulate why downward movement of the lowland block occurred under conditions favoring sedimentation but not favorable for formation of warped structures, whereas later downward movement of the same block occurred during conditions favorable to warping, but unfavorable to sedimentation.

One possible tectonic event that could influence the mechanics and for which ample field evidence exists is a change of dip of the border fault. We know that the Triassic strata, which were originally horizontal or even slightly inclined initially to the west, have been tilted eastward as much as 15° to 30° (locally more); such crustal movement must have also affected the attitude of the border fault. Hence, if the two periods of faulting in question were separated by a time of regional arching along a northwest-southeast-trending axis, then the dip of the border fault would not have been the same in each episode. During the episode of movement and Triassic sedimentation, the dip of the border fault must have been steep—vertical to possibly steeply dipping to the southeast (a reverse fault)—whereas during the second episode of faulting the dip would have approached its present value of 50° to 60° westward (a "normal" fault). Furthermore, the intervention of a period of significant northeast-southwest-trending regional arching could have altered the topography and drainage of the former lowland area so thoroughly that the streams might have drained away from the former lowland block during the second episode of faulting. This change might explain the absence of sedimentation in the former lowland block during the second episode of faulting.

(2) The two episodes of supposed downward movement may be illusory and only conditioned on the present total net displacement, which is relatively downward on the lowland block by the total thickness of Triassic sediment preserved. *All* fault displacements, however, need not have been downward to produce this net effect. During sedimentation we know that the lowland block moved downward because it served as a sediment trap, but the lowland block may have moved *upward* during the second episode of movement when the warped structures formed and no further sediment accumulated on it. A net displacement downward would still exist if this upward movement of the second episode failed to exceed the earlier amount of downward movement.

If it is supposed that the warped structures formed by upward movement of the lowland block and that Wheeler's theory of the relationship of the fault surface to warped structures is correct then, in order to have influenced the warped structures, the border fault must still have dipped southeastward. Origin of the warped structures on an upward-moving lowland block against a southeast-dipping border fault would explain

the systematic plunge of the warped structures toward the border fault; these plunges are otherwise not understood. In this interpretation the known regional arching can have occurred only *after* formation of the warped structures; these structures were rotated to an eastward-dipping attitude after their formation but before their displacement. The existence of steeply dipping faults at the western margin of the block on which the warped structures are located in Connecticut stands in opposition to this sequence, for these faults probably formed essentially with their present dip and have not been rotated. According to my interpretation, they were in existence when the warped structures formed; if so, then the second alternative requires that they must have also been rotated to the west when regional arching occurred, and this seems unlikely.

Wheeler's hypothesis of the relation of the warped structures to the border fault, which is accepted here, thus seems scarcely applicable to the second alternative. Furthermore, comparison with the warped structures along the Mexia-Talco fault zone in Texas, which fit Wheeler's concept, and the conditions implied in the second alternative indicate that anticlines should be located where synclines occur and synclines should be found in the place of present anticlines. For these reasons the second alternative is rejected, although it is the only alternative that seems to account for the axial plunge of the warped structures.

(3) A third possibility consists of a modification of the second alternative, in that the dominant mechanism of the second episode of faulting may have been strike-slip instead of vertical movement. Strike-slip movement, for example, might be invoked to explain why the warped structures originated without further sedimentation on the lowland block, for under such an arrangement no topographic lowland needs to have formed.

The difficulty with the strike-slip interpretation rests with the shape and orientation of the warped structures with respect to the border fault and also on the close spatial relationship of clasts in the Triassic sediments to potential source areas in the adjoining pre-Triassic terrane of the Eastern Upland block. The warped structures are essentially symmetrical, their axial planes are vertical, and their axes are perpendicular to the border fault. The potential disposition of these warped structures in a strike-slip setting depends on the larger force field that gives rise to the strike-slip faults. In any case, however, the structures might be expected to be systematically asymmetric with axial planes possibly dipping in the direction from which active motion came. The systematic distribution of en echelon tension fractures and certain compressional structures in the diagonal direction with respect to strike-slip faults suggests that fold axes might be oriented, not in the perpendicular position, but at 45° to the border faults under certain conditions of strike-slip movement (Longwell, 1960).

The strike-slip interpretation is opposed also by such evidence as is available regarding the distribution of clasts in the Triassic sedimentary rocks and potential source areas in the Eastern Upland block (Krynine, 1950; Eaton and Rosenfeld, 1960). Clast studies indicate that in Connecticut little if any lateral displacement has occurred on the border fault. In central Connecticut, for example, a distinctive blue tourmaline is distributed in a fanlike arc away from a point source in the Eastern Uplands east of Middletown opposite the apex of the fan (Krynine, 1950).

None of the difficulties encountered by the second and third alternatives affects the first alternative, which remains the most likely.

Structural history after origin of warped structures. The remainder of the structural history—that of displacement of the warped structures by the lowland fault system (third-order faults) and later injection of dikes into these faults—is not influenced by the theory adopted to account for the warped structures.

Summary of structural history. From the foregoing discussion I propose the following synthesis of structural history:

(1) Steep-dipping normal (or possibly even reverse) faults initiate the Triassic lowland. In Connecticut, the Eastern Upland block moved intermittently upward, whereas the Central Lowland block moved intermittently downward. Regional drainage was from upland to lowland so that the lowland block became a site of sediment accumulation.

(2) This episode of downward movement and sedimentation ended when a regional northeast-southwest-trending arch arose in the midst of the former lowland. As a result of arching, the strata were tilted eastward in Connecticut and the steep-dipping border fault was rotated to its present westward-dipping position. Also, the topographic lowland disappeared and the formerly interior drainage changed to an external drainage network.

(3) Second-order faulting took place along the now tilted border fault; new, relatively steeply dipping, second-order faults were also formed. The warped structures originated by differential subsidence on graben blocks outlined by the moderately dipping border fault and steeply dipping second-order faults, but no lowland formed and the drainage no longer was interior.

The steep second-order faults, some of which dip southeast, suggest that no significant regional arching has occurred since their formation. This further supports the temporal sequence of doming before second-order faults.

(4) The essentially vertical, third-order lowland fault system originated. Movement caused offsets of the warped structures, but no further warped structures formed.

(5) Dikes were injected along the steep third-order faults (possibly also in the steep second-order faults).

Regional structural implications. The history of structural events outlined here from a small area in southern Connecticut also applies to all other areas in the Connecticut Valley Triassic belt of Connecticut and Massachusetts. In central Massachusetts, particularly, clear evidence is found that the pre-Triassic basement is involved in a warped anticline near Amherst. The basement can also be observed to have been involved in other warped structures and was probably active in all.

In southeastern New York and northern New Jersey a similar history can be traced up to the point where the warped structures formed. The only differences from Connecticut are that in New York and New Jersey the regional dip is to the northwest and the border fault is found on the northwest and now dips southeast; as in Connecticut, the axes of warped structures also strike northwest-southeast. The New York-New Jersey Triassic belt clearly lies west of the northeast-southwest-trending regional arch.

A remarkable coincidence of structure occurs at the south end of the Connecticut Valley belt in New Haven harbor and at the northeastern end of the New York-New Jersey belt north of Stony Point, west of Peekskill, N.Y. In both places the base of the Triassic strikes into the border fault, as near Amherst, Mass. Near Stony Point, N. Y. the dip of the basement surface is toward the southwest, whereas in New Haven the dip is toward the east-northeast, forming perfect mirror symmetry and suggesting that both are parts of the same larger structure—a regional anticline—that strikes northwest-southeast and extends from the border fault in Connecticut to the border fault in southeastern New York (FIGURE 1). The name Danbury anticline is suggested for this structure.

The Danbury anticline crosses the earlier-formed, northeast-southwest-trending regional arch (Barrell's "Taconic geanticline") near Danbury, Conn. This intersection coincides with a "terrace" of high positive anomalies (35 milligals) in the regional gravity map (Longwell, 1943), but the control network becomes very sparse in the area of most interest here. The gravity "high" is explained as a result of warping of the entire thickness of the earth's crust, so that the subcrustal material approaches the surface more closely here than elsewhere.

Assuming that the Danbury "anticline" exists as is here proposed, one might ask the further question of what happens southwest of it and southeast of the northeast-southwest-trending regional arch. If the broad-terrane hypothesis for the Triassic is correct, as I believe, then a belt of Triassic rocks should be present under western Long Island or New York Bight and possibly also under the New Jersey Coastal Plain. Confirmation of the existence of such an area of buried Triassic rocks under Long Island is evidently provided by Wheeler's (1938) account of an earlier unpublished report by W. O. Crosby on the geology of Long Island, which included the log of a deep well at Duck Island northeast of Northport and southeast of Eaton Neck in northwestern Suffolk County (FIG-

URE 1). According to Crosby, the well entered Triassic (?) redbeds at a depth of 602 feet and penetrated 1048 feet of such rock before drilling ceased at a depth of 1650 feet. The idea that Triassic rocks exist under Long Island, however, has not been generally accepted by later workers (deLaguna and Brashears, 1948; Suter *et al.*, 1949).

Information on the bedrock below the Coastal Plain cover is provided by wells drilled to bedrock in western Long Island and by several seismic traverses that have been made both on land and from ships along lines in eastern New Jersey, south of New York City, and across Long Island Sound (Ewing *et al.*, 1939 and 1940; Ewing *et al.*, 1950; Oliver and Drake, 1951; Carlson and Brown, 1955). The wells and seismic profiles limit the areas within which any buried Triassic rocks might be present, but they do not yet exclude the possibility that buried Triassic rocks exist under Long Island and vicinity.

Summary

The structural history of the Triassic rocks of central Connecticut is based on interpretation of the stratigraphy and present attitudes of the strata. Boulder conglomerates adjacent to the north-northeast-south-southwest-trending border fault testify to syndepositional movement on the border fault during the Upper Triassic. After deposition, the strata were tilted to the east-southeast and also warped into a series of anticlines and half basins whose axes strike northwest-southeast and plunge toward the border fault. These warped structures, which involve the underlying pre-Triassic "basement," are limited to elongate blocks that are bounded on each side by a fault; they have also been displaced by another set of faults (lowland fault system) into which have been injected late basaltic dikes.

The Connecticut Valley belt of Triassic rocks ends on the south in New Haven harbor where the pre-Triassic "basement" surface at the base of the Triassic, dipping steeply east-northeast, strikes directly into the border fault.

A similar history is indicated for the New York-New Jersey belt of Triassic rocks but with dip of strata toward the northwest into a southeast-dipping border fault. The northwest-dipping New York-New Jersey belt and southeast-dipping Connecticut Valley belt together define a regional arch whose axis crosses western Connecticut.

The New York-New Jersey belt of Triassic rocks ends north of Stony Point west of Peekskill, N. Y. where the pre-Triassic "basement" surface, dipping southwest, strikes into the border fault.

The mirror symmetry of the surface of the basement at the southwestern end of the Connecticut Valley belt and northeastern end of the New York-New Jersey belt defines a major northwest-southeast-trending regional anticline whose axis is parallel to those of the other warped structures in the Triassic rocks of both areas and that extends from the

border fault in Connecticut to the border fault in southern New York. This Danbury anticline (new name) intersects the northeast-southwest-trending regional arch near Danbury, Conn. in an area where positive gravity anomalies of 35 milligals have been found.

The reported occurrence of Triassic rocks in a deep well under the coastal plain cover of Long Island is explained as a remnant of a southeast-dipping belt that lies east of the northeast-southwest-trending regional arch and southwest of the Danbury anticline. The absence of Triassic rocks under Long Island Sound, indicated by seismic traverses, is explained by the existence there of the broad crestal area of the Danbury anticline.

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SECTION OF BIOLOGICAL AND MEDICAL SCIENCES

ON THE ECOLOGY OF ESTUARINE ZOOPARASITES*

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The ecology of animals parasitic upon estuarine hosts has not been discussed previously as a unit of study, although there is ample supporting literature on the ecology of parasites. Several papers and books (Baer, 1952; Caullery, 1952; Foster, 1960; Hopkins, 1957; Huff *et al.* 1958; and Otto, 1958) were consulted in preparing this introductory consolidation of some knowledge and conjecture upon estuarine parasites.

The Estuarine Environment

An estuary is a coastal embayment where tidal waters undergo measureable changes in salt concentration, either through run-off or evaporation. In addition to these changes in salinity, other environmental conditions also may vary widely. The effects of these environmental fluctuations are readily recognizable, for example dessication during low tides; distribution and buoyance as moderated by tidal and wind currents and density of water; salinity on water conservation (osmoregulation); penetration, intensity, and duration of light upon metabolism; and nutrients and siltation upon growth. These environmental fluctuations, in terms of their specific action upon organisms, may: be lethal; control, limit, or impose an accessory load upon metabolism; mask or supplement each other; and direct the responses of the organisms (Fry, 1947).

An analysis of world-wide distribution of organic production by plants (Odum, 1959) shows that estuaries may rank among the most productive areas. This high productivity by estuarine plants provides a large food source for the few animal species that are able to survive estuarine conditions. Under these conditions relatively large animal populations abound in estuaries.

Synopsis of Parasitism

Some of the intriguing problems in delineating the ecology and evolution of parasities are concerned with the role of adaptation and with the phenomenon of parasitism (Huff *et al.*, 1960).

The role of adaptations. Parasites have diverse origins among many phyla, but they all have in common their dependence upon a host or hosts. Some of the adaptations that enable a parasite to complete its life cycle are those concerned with transmission and reaching, entering, and

*This paper, illustrated with slides, was the first of three papers presented at a meeting of the Section on November 14, 1960. Contribution No. 19, University of Delaware Marine Laboratories.

surviving on or in the host, including adaptations at the trophic, reproductive, and genetic levels. Assumption of the parasitic way of life has evolutionary consequences such as isolation and adaptive modifications.

The phenomenon of parasitism. Since the essence of parasitism is the interaction of two living organisms, the consequences to each must be considered, including; types of symbiotic associations; specificity of the host-parasite relation; effect on the evolution of the host species; parasitism at subcellular, cellular, organismic, and social levels; effects of parasitism upon individuals, populations, and communities; and the action and reaction between parasite and host (both injurious and beneficial results).

Ecology of parasites similar to other animals. Competition or cooperation between individuals and populations within a community are related to the demands for *"Lebensraum,"* "metabolic fuels," and reproduction. There are several intergrading categories, from "neutralism" to "parasitism" in which animals interact in obtaining space and food (Odum, 1959). Estuarine animals can obtain certain of their "metabolic fuels" from a more or less common pool of dissolved gases and organic nutrients; however, the trophic situation is absolute: all animals are consumers. We can consider that the ecology of parasites at the individual, population, or community level is similar to the ecology of any other consumer group, although parasites generally are more intimately dependent upon other members of the community, either by "choice or experimentation" (usually a facultative relationship) or because of a high degree of adaptation or specialization (obligatory).

The Oyster Bed Community

Recent advances in the study of natural assemblages of organisms – that is, of communities (Odum, 1959; Odum, 1960) – provide a framework within which to examine the role of parasitism. These advances warrant the selection of a community for discussion here, although comment upon community studies is beyond the scope of this article.

Among estuarine communities, ample shelter and several trophic possibilities are available in the reeflike substratum provided by the oyster *Crassostrea virginica* Gmelin. It is not surprising, therefore, to find among the assemblage of organisms that comprise the oyster bed community many taxa of animals that are linked together as predators or hosts in life cycles of parasites.

Larval survival of estuarine species. The ability of free-swimming larvae of marine trematodes to survive wide ranges of salinity and to remain infective (Stunkard and Shaw, 1931) will undoubtedly be found to be the case for other species found in estuaries. However, although the survival of any species is determined by climatic and substrate conditions, biotic processes, through the toll levied upon the larval stages by en-

emies, may be the most critical barrier to survival (Thorson, 1950). In this, parasites should be no different from other estuarine species that also have planktonic larvae and produce large numbers of eggs.

Besides consuming large numbers of bivalve larvae, filter and/or mucus-net feeders may levy a heavy toll on larval parasites. Examples of these feeders are the blue mussel *Mytilus edulis* L. (Thorson, 1946); the comb jelly *Mnemiopsis leidyi* Agassiz (Nelson, 1925); and the mud-blister worm *Polydora ligni* Webster. The latter, when numerous, may overstep its "commensal" habit and become a menace to the entire oyster bed through copious and ultimately "suffocating" masses of sediment and faeces entangled in their thick mucus secretions (Nelson and Stauber, 1940).

Another aspect of the survival of parasites should not be overlooked: the role of parasites and hyperparasites in causing fluctuations of estuarine populations, ranging from a more or less static drain upon a population through a decrease in the health, vigor, or escape reactions of infected individuals to mass mortalities (Hopkins, 1957).

Hyperparasites. Since a haplosporidian has been implicated (Haskin, 1960) in the recent heavy mortalities of oysters in Delaware Bay in 1957, it is of interest to note that some haplosporidia are hyperparasites of marine species occurring in gregarines infecting polychaetes (Stubblefield, 1955) and in the sporocysts and cercariae of *Bucephalus* sp.-infecting oysters (Mackin and Loesch, 1955).

Oysters, Nematopsis, mud crabs, and toadfish. Mud crabs, which feed upon young oysters and barnacles, are a principal item in the diet of *Opsanus tau* (L.) the toadfish (McDermott and Flower, 1953). Some mud crabs are parasitized by rhizocephalids (Reinhard and Reischman, 1958); others are hosts for a gregarine, *Nematopsis ostrearum* Prytherch (Sprague and Orr, Jr., 1955). Vegetative stages of this gregarine parasite develop in mud crabs, spore stages in the oyster. A dynamic equilibrium is reached in the oyster between elimination of and reinfection by the parasite (Feng, 1958). Elimination by migration of the sporozoite-laden amoebocytes from the oyster may be more effective than intracellular digestion of the invading parasite (Stauber, 1950). Hence a build-up of the number of spores in the oyster may be largely a measure of the "success" or survival of the stages in the mud crab. The high levels of infections that occur in the oyster may be enhanced because the gregarious habit of the crabs provides for recurring contacts.

"Predator-prey" recognition. Chemostimulation of predators and of commensals, as well as the "escape" response of prey, has been studied (see reviews by Davenport, 1955; Passano, 1957), but a specific chemotactic stimulus may or may not exist for certain host-parasite combinations (Sudds, Jr., 1959). The epitome of the "predator-prey" recognition "set" may well be host-specificity for parasites at the intimate biochemical-nutritional level.

The host as an environment. In addition to phagocytosis and biochemical attributes of the host, mixed infections and physiological cycles should be studied to see if these affect parasites. The oyster respires anaerobically during periods of shell closure. Since shell closure follows daily, monthly, and 27-day cycles of activity (Brown, Jr., *et al.*, 1955), a "cyclic" anaerobic environment probably exists for oyster parasites.

Small crustaceans, squid, and elasmobranchs may be linked in trophic and parasitic chains in oyster bed communities; therefore, the penetration of material of potential nutritional or osmoregulatory value from the gut of elasmobranchs into the tissues of tapeworms constitutes a class of active or passive phenomena of first importance in parasitism (Read *et al.*, 1960; Simmons, Jr., *et al.*, 1960). Comparison of the assimilation by subcellular and cellular parasites could well demonstrate additional metabolic pathways.

Conclusion

Parasites subjected to the same environmental forces as other organisms presumably are affected in much the same manner. It is only at the more intimate host-parasite level that differences may occur.

The effects of the estuarine environment upon host species also imposes, indirectly, another set of conditions under which parasites live.

Among several approaches to the study of estuarine zooparasites are two more recent research methods that offer intriguing possibilities: trophic analyses of communities (Odum, 1960) and the suggestion (Huff, 1956) that laboratory investigations on appropriate host-parasite combinations would help to elucidate parasitological phenomena.

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THE EVOLUTION OF SALINITY TOLERANCE IN FUNGI*

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Whether an organism is or is not considered marine depends to some extent upon the definition used in delimiting categories. One method of classification depends upon reaction in laboratory culture. Thus, if fungi must be strictly stenohaline to be considered marine types, few can qualify. If, however, they show their most vigorous development under laboratory conditions simulating those found in the sea, others can be included. Finally, if they need only to be able to grow and reproduce – albeit with reduced efficiency – under marine conditions, then still more might be counted.

A different method of cataloging is to judge an organism by the place in nature where it occurs. Thus one might include as marine organisms those that have such a range of tolerance that they can live in a wide variety of environments, including that of the sea. A similar manner of classification of parasites was arranged by using the terms obligate and facultative, although many a fungus can thrive on either living or dead matter, and the most fastidious of the obligate parasites may yield to artificial culture methods. In the latter case, an obligate parasite is simply one that is always found as a parasite in nature. As parallels for marine mycology, obligate marine fungi would be those morphological types known only from the sea; facultative ones would be those found both in the sea and in nonmarine habitats.

The strictest definition of a marine organism would admit only stenohaline types or those that are always found in the sea, although they may be restricted to that habitat because of factors other than salinity. For the purposes of the present discussion, however, a fungus is considered marine if it is obtained from marine or brackish water and if it can grow and reproduce on media containing 35 parts per thousand of such salts as occur in ocean water. This classification admits euryhaline strains and those with morphologically similar terrestrial or freshwater relatives.

A special difficulty in the study of microorganisms is the chance that a presumably marine type is a contaminant or a fortituous and temporary dweller in a single place. In some instances neither possibility can be completely ruled out but, if maximum precautions in collecting are taken and if subsequent culture methods show that the organism can grow in saline media, the likelihood of either possibility is minimized.

*This paper, illustrated with slides, was the third of three papers presented at a meeting of the Section on November 14, 1960.

The second of three papers presented at this meeting of the Section, "The Associations of Fungi with Marine Animals," by T. W. Johnson, Jr. of Duke University, Durham, N.C. will be published in a subsequent issue of the *Transactions*.

The isolation and description of increasing numbers of fungi from saline habitats during recent years (Johnson and Meyers, 1957) raises questions as to the origin of their ability to survive in or even to require a marine environment. In considering possible routes by which fungi have come to possess salinity tolerance, some assumptions and some facts must be made explicit. The assumptions are the Darwinian ones that similarities indicate relationships and that the place where the greatest number and diversity of species and genera occur is the place of origin of the class. If marine Ascomycetes, for example, are assumed to be closely related to terrestrial Ascomycetes because of morphological similarities, we can assume further that either the terrestrial ones gave rise to the marine ones or vice versa. In view of the fact that the number of terrestrial species is greater – indeed much greater – than the number of marine ones, the conclusion must be reached that the marine Ascomycetes evolved from the terrestrial ones.

A recent review of the taxonomic groups of fungi with marine representatives (Wilson, 1960) points out that every major division includes sea-dwelling members. The Basidiomycetes are an outstanding exception in that they are poorly represented, but even they occasionally grow in places where salinity is a considerable factor (Ritchie, 1946; Feldmann, 1959). Notable, however, is the fact that in all groups the greater number of species and genera is terrestrial.

An increasing tolerance of saline conditions should have left, during the course of its development, some evidence in the form of strains with intermediate tolerance. Such do indeed occur in many of the fungus groups, as for example in the marine slime molds (Young, 1943), the Phycomycetes (Wilson, 1960), and the Ascomycetes (Gustaffson and Fries, 1956). Transitional forms have been demonstrated in most detail in the Fungi Imperfecti, most of which are, from the standpoint of natural relationships, Ascomycetes. These organisms exhibit a diversity of reaction to salinity ranging from definite intolerance in some strictly terrestrial species, that is, those collected in terrestrial habitats, through what might be called a grudging tolerance in some *Alternarias*, to a positive "preference" for a marine degree of salinity (especially at some incubation temperatures) in marine *Curvularias* (Ritchie, 1959*a* and *b*; also unpublished observations).

Special morphological adaptations to a marine existence are not common, although special spore forms are produced by a few Ascomycete genera (Meyers, 1957; Wilson, 1954). Slight but measurable differences in cell size can be produced by variation in incubation temperature and in salinity of medium in a marine *Phoma* (Ritchie, 1961), but whether such differences have any significance in the evolution of new species with ocean-dwelling tendencies is not known. By and large, however, fungi that live in salt water do not differ markedly in structure from those of fresh-water or terrestrial habitats.

In the absence of a well-defined morphological evolutionary series in in any of the fungus groups, speculation as to the derivation of the marine habit is necessarily based on other facts. In the present incomplete state of knowledge of these plants, we can draw tentative conclusions from distribution and numbers of species and from cultural characteristics. From such information, it seems likely that those species and genera that now inhabit salt water are derived from fresh-water forms either directly or indirectly by way of terrestrial intermediate ancestors. Marine Phycomycetes furnish an example of the former, and marine Ascomycetes and imperfect fungi exemplify the latter.

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DIVISION OF ENGINEERING

ENGINEERING EDUCATION: PRESENT AND FUTURE*

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Engineering Education: Present and Future

In an era when the United States—and the world—presents a great challenge to able young people to study engineering, engineering schools are confronted with the paradox of decreasing enrollments. Freshmen enrollment in engineering colleges in the United States declined by 11 per cent in 1958 and 3.3 per cent in 1959. Of course there are a handful of schools, such as Massachusetts Institute of Technology at Cambridge, Mass., and California Institute of Technology at Pasadena, Calif., that are confronted with more applicants than they can enroll.

Whatever the reason, whether it is the glamour associated with science and research or the type of opportunities available through advertisements typified by those in *The New York Times*, the most able potential engineering student is, instead, pursuing major studies in physics or mathematics. There is no desire to minimize the importance of these two fields and the great impact they have made on modern science. However to continue to produce theoreticians without a parallel development of men and women who are educated to translate science into useful human needs—which of course is engineering—is to deter the progress of a country whose economic evolution has come largely from the constant development of its technology.

In view of the need for more science in the basic education of an engineer, engineering educators are faced with two separate problems. One is the continued insistence on a four-year undergraduate curriculum, from which some graduates are presumed to be competent to enter professional practice. The other is the increasing demand for more science content in the undergraduate curriculum and the consequent reduction in application or professional courses. The agility with which a particular engineering school can revise its engineering curricula from one that has been "practical-oriented" to one that is "science-oriented" is largely a function of the capabilities and attitudes of its faculty. Much could be said here about faculty reactions to proposed revisions in curricula, but such a discussion would require volumes. Nevertheless, whatever the intent or goal of such discussions might be, the obvious fact is that it is becoming increasingly difficult, if not impossible, to graduate an engineer in four years who, without additional study of a long internship, is equipped to enter the field of professional engineering.

*This paper was presented at a meeting of the Division on November 18, 1960.

Within the past decade, a definite trend toward science-oriented engineering curricula has been established. Undergraduate curricula in electrical engineering and chemical engineering tend more and more to become science-oriented. Perhaps the more abstract nature of these fields requires greater theoretical treatment than other fields. The "exploding" advances in all of the basic and applied sciences have created the need for a more thorough understanding of these sciences by the engineer who must utilize them in the creation of new products. At the moment many of these products—the term is used to encompass the whole gamut of articles from small electrical components to missiles and structures—are concerned with national defense. There is no doubt, however, that the evolution of consumer goods and services, because of economic competition alone, will demand a high degree of scientific and technical competence, In effect, what once was accomplished solely by intuition and empiricism is now being replaced by abstraction, abstraction based on the utilization of modern chemistry, physics, and mathematics.

The end result of four-year undergraduate curricula in engineering, which has incorporated more engineering sciences to the exclusion of design and application courses, is a graduate oriented more toward developing his skills in analysis rather than in engineering design.

If such a four-year graduate endeavors to enter the professional fields, he requires a lengthy period of adjustment of indoctrination so that he may understand the practical concepts of design, construction, production, and distribution. Industry, having enormous facilities and wealth, can absorb the cost in money and time necessary to provide an internship for the four-year graduate. The question arises, however, whether such a graduate involved in the various specialized areas of industry really is a technician rather than a professional engineer, with all of the implications that the latter term implies.

Perhaps the distinction between "technician" and "engineer" is an illusive one under any conditions. Nevertheless the term professional engineer has been, in recent years, descriptive of those engineers who assume a legal as well as an ethical obligation for the accuracy of their works, Herein lies the "great divide" between engineering educators. The professional-minded educator feels the necessity for "professional" courses somewhat analogous to those given by the medical schools. The science-minded educator believes, because of background or desire, that a large part if not all of an undergraduate engineering curriculum must be science-oriented. In fact, this divide has evolved within the engineering school a separate curriculum devoted exclusively to the engineering sciences. The shift in enrollment to the latter curriculum on the part of more competent students has resulted in a loss of such students to the more standard engineering curricula. This shift is especially true in the case of civil engineering, a field in which schools with high admission standards find a noticeable lack of students.

At the Cooper Union, where admission is based solely on high scholastic ability, it is extremely difficult to find 25 students who wish to study civil engineering and have entrance scores that will permit them to compete successfully with the remaining 75 electrical, chemical, and mechanical engineering students. The Cooper Union admits only 100 freshmen into its day session and these are equally divided among the four engineering curricula. This dearth of prospective civil engineers exists in spite of the fact that the civil engineering curriculum has been revised to include a greater content of the associated engineering sciences. Within the last year, The Cooper Union with the co-sponsorship of the American Society of Civil Engineers, New York, N. Y. and the American Society for Engineering Education, Urbana, Ill., completed its study under a grant from the National Science Foundation, Washington, D. C., to appraise current and proposed programs for the content of curricula in civil engineering. The proposal to apply for the grant from the National Science Foundation was conceived by The Cooper Union and further recommended by a group of 10 educators that met in June 1959 under the sponsorship of that institution. The grant provided for two preliminary planning conferences of three days each, held at The Cooper Union Engineering Camp at Ringwood, N. J. These two conferences were attended by 30 professional civil engineers and educators representing various areas and schools throughout the nation. The study culminated in a conference at the University of Michigan, Ann Arbor, Mich. attended by 250 engineers and educators representing all states in the union including Hawaii and Alaska.

The recommendations of the conferences of the first two planning sessions may be summarized in the following resolution, unanimously adopted for presentation at the Michigan conference:

"WHEREAS the report of the ASCE Task Committee on Professional Education published in 1958 revealed that a large proportion of the membership believes that contemporary civil engineering education falls far short of meeting contemporary needs, and

WHEREAS two-thirds of the membership favors extending the period of undergraduate study to five years, and

WHEREAS the work of the civil engineer is becoming increasingly interwoven with and vital to man's economic, political, and social well-being, and

WHEREAS modern technological developments are ever widening the vistas of civil engineering and deepening its scientific roots,

WHEREAS these trends are constantly accentuating and creating needs that can be met only by truly professional people whose education has the breadth of a liberal education and the depth of a firm foundation in mathematics and science, and

WHEREAS professional education can fully realize its objectives only

as it can grow out of the breadth and depth of pre-professional studies, and

WHEREAS a learned profession will grow and prosper in proportion to the creativity and vision engendered by continuing research and study at its frontiers,

THEREFORE BE IT RESOLVED ... that this conference favors the growth in universities and colleges of a pre-engineering, undergraduate, degree-eligible program for all engineers, emphasizing humanistic-social studies, mathematics, basic and engineering sciences with at least three quarters of the program interchangeable among the various engineering curricula; to be followed by a professional or graduate civil engineering curriculum based on the pre-engineering program and leading to the first engineering degree, with a civil engineering degree awarded only at the completion of the professional or graduate curriculum, and

BE IT FURTHER RESOLVED ... that increasing opportunities should be provided for qualified students, by progressively more advanced study and research, to earn graduate degrees at the Master's and Doctor's level, and

FINALLY BE IT RESOLVED ... that all engineering societies in ECPD and EJC be advised of this resolution and be urged to support it by concurrent resolutions in order that unified action will be taken to secure a truly professional educational base for professional engineering practices."

The agenda for the three-day conference at the University of Michigan was evolved by planning a series of papers and discussions intended to emphasize the urgent need for implementing the foregoing resolution. Although the delegates at the conference agreed unanimously on the theme of the conference—which in essence was delineated by the foregoing resolution—there was reluctance to accept the resolution itself. In view of the fact that such an acceptance on the part of an individual delegate could be construed as binding the delegates' institution to the resolution, this reluctance was understandable. Consequently, the conferees did agree to study the proceedings of the conference and vote by ballot at a later date on the resolution.

The interesting fact coming out of this study was the general acceptance of the need for a period greater than four years in order to provide a program leading to the first undergraduate professional degree. Generally this period was considered as five years. Three of the first four years would provide a pre-engineering curriculum—to apply to all engineering departments—comprising: mathematics (15 or more semester hours); humanities, social studies, English, and speech (24 or more); physics, including modern physics (12); thermodynamics and heat transfer (6); statics, dynamics, and strength of materials (9); fluid mechanics (4 or more); graphics (3 or more); natural sciences (3 semester hours);

measurements and data processing (3); and modern engineering materials (3). The remaining year of the first four years would include the required fundamental courses in any area of engineering and elective courses. The four-year program would lead to the undergraduate B.S. degree.

More capable students could then go on to the fifth year to complete professional-oriented courses leading to appropriate professional degrees such as M.C.E. and M.E.E. Students showing greater abilities for more complex studies in a professional-oriented curriculum could go on to a sixth year and professional degrees such as C.E. and M.E. An additional year could lead to the degree Doctor of Engineering, which would create an outstanding group of engineers most able to cope with highly complex problems of current and future technology.

Sufficient flexibility is provided the transitional year of the first four-year program to permit the research-minded student to choose science-oriented rather than professional-oriented terminal courses. The fifth, sixth, and seventh years could then be devoted to a program leading to the Ph.D. under the jurisdiction of the presently conceived graduate school.

To imply that the study initiated by The Cooper Union was the first single step taken to redefine an educational goal within the engineering fraternity would be grossly misleading indeed. A task committee was appointed by The American Society of Civil Engineers to report on the areas and scope of civil engineering education. That committee's report,[1] although inconclusive with respect to unanimous agreement on a specific curriculum, did state " ... it is imperative that the Civil Engineering profession begin now to redefine and reorient the education processes to meet the needs of the present and the challenge of the future. Improvement in the quality of professional education is the essential first step in enhancement of professional status."

Studies in curricula other than civil engineering have been made within recent years, and these have resulted in reorganized and reoriented programs in specific areas. Perhaps the most outstanding of recent studies in engineering education is the "Report on Evaluation of Engineering Education"[2] prepared by a committee of the American Society for Engineering Education (ASEE). This committee recognized the need for "Professional-General" and "Professional-Scientific" as descriptive of two separate curricula that might exist in different schools, depending chiefly on staff and facilities. The details and implications of that report were many and varied. One might add, too, that the reactions of both the professional and educational engineering fraternity were many and varied. The underlying theme or philosophy that guided the committee may be best illustrated by the following statement, which appeared as a summary to the report: "Engineering Education must contribute to the development of men who can face new and difficult engineering situations with imagination and competence. Meeting such situations invariably involves both

professional and social responsibilities. The committee considers that scientifically oriented engineering curricula are essential to achieve these ends."

In 1956, one year after the ASEE Report[2] had been published, Warren E. Wilson, Mudd College, Calif. made a study[3] to determine the reactions of members of ASEE to a proposal for a single curriculum in engineering that in effect would replace all specialized curricula in the various engineering branches. Except for 18 credits devoted to engineering analysis and design, the remainder of the 136 total credits were in the area of engineering sciences, basic sciences, humanities, English, and drawing. Fifty per cent of the academic and nonacademic community questioned indicated that specialized engineering curricula would be decreasing in number or eliminated within the next 10 years. On the whole, the study showed that members of the nonacademic community (such as engineers in industry) were more strongly in favor of the single curriculum than the educators. Most interesting is the fact that a relatively large percentage of educators were at all interested in such a radical proposal.

Within past weeks, Johns Hopkins University at Baltimore, Md. announced its plan to reorganize the departments of Aeronautical, Civil, and Mechanical Engineering into a single Department of Mechanics. Here again, separate undergraduate engineering curricula have been abolished in favor of the more fundamental curriculum of mechanics.

If, as all trends seem to indicate, the first three or four years of a four-year program will be devoted to a single uniform curriculum, heavily weighted—if not almost exclusively so—in the area of basic sciences, little doubt remains as to the need for a longer period in order to educate a professional engineer. Perhaps the all-important question is how this best can be accomplished. Most privately endowed tuition schools are already hard-pressed financially. To impose professional curricula in established graduate departments would create an additional financial hardship in terms of additional faculty and space. If such universities proceed to establish a uniform single undergraduate curriculum in engineering, which could appropriately be designated as pre-engineering, then professional engineering education must come from schools devoted exclusively to the education of professional engineers, analagous to the existing schools of law, pharmacy, and medicine.

It is possible at the moment to envisage a four-year professional school built upon the first two years of pre-engineering given by many junior and community colleges. Certainly schools such as the University of Califomia, Los Angeles, Calif., which can draw on the many junior and state colleges in the state, could very easily eliminate its first two years of engineering education and devote itself entirely to professional and graduate school engineering education. The State of New York is currently reacting to the tremendous demand for public education beyond high school by constructing both state and community colleges. It would

be impossible to engage professionally qualified faculty members in all of these undergraduate schools that hope to incorporate typical engineering curricula in their program. From sheer necessity it appears that junior, community, and state colleges must concern themselves with only the pre-professional and terminal education programs.

In conclusion, it appears essential, either within the existing framework of the University or separately, that truly professional schools of engineering be established. These schools can evolve four-year programs leading either to professional degrees such as C.E. or M.E. or such as the D.C.E. or D.M.E., depending upon whether the entering student comes from two- or three-year pre-engineering curricula. Faculties for such professional schools of engineering can be drawn from men eminently qualified both by education and experience, somewhat in the fashion of the European schools. The legal authority to grant the professional degrees should lie only within the domain of the State Boards of Registration composed wholly of engineers. There is no intention on my part to assume that this concept of professional engineering education is new or novel. However this period of advancing science and technology, having great need to educate engineers who can cope with the increasing complex problems involved in design, construction, and production, provides the the best opportunity in the history of engineering to embark on the establishment of truly professional schools of engineering.

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DIVISION OF MICROBIOLOGY

THE EFFECT OF TUMOR SUSPENSIONS ON HUMAN RED BLOOD CELLS*

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Previous studies [1-3] in our laboratory with tumor-bearing mice have shown the existence of two different hemolytic responses independent of each other. In one, the hemolytic behavior of red blood cells from tumor-susceptible C3H mice is different from that of red blood cells from tumor-resistant C57 mice. This difference consists of a far greater tendency of hemolysis of the erythrocytes of normal C3H mice when incubated with dilutions of rabbit serum than of the red blood cells from C57 mice. On the other hand, the red cells from tumor-bearing C3H mice are relatively resistant to hemolysis and tend to react like the red blood cells from C57 mice.

In the other hemolytic response, saline suspension of breast tumors from C3H mice hemolyze red blood cells from both C3H and C57 mice.[4] When the same tumor suspensions are employed, erythrocytes from tumor-bearing C3H mice show limited hemolysis or none at all. Inhibition of hemolysis is affected by the growth behavior of the tumor and by the stage of tumor growth. It is more often observed with fast-growing rather than slow-growing tumors and with tumors in the early and moderately advanced stages of growth rather than in the far-advanced stages.

On the basis of these findings an investigation was started on the hemolytic activity of human tumor suspensions and their effect on red blood cells from cancer patients and from healthy donors. It was observed that high speed supernatants obtained from human pulmonary carcinomas had a hemolytic and/or clumping effect on human red blood cells.[5] More recent studies confirmed our earlier findings that only a few lung-cancer supernatants revealed a direct hemolytic effect. In a two-step procedure described below it became evident, however, that a great many supernatants derived from human pulmonary carcinomas damaged human red blood cells, although hemolysis was not obvious on first contact. It was also seen that cancer high speed supernatants that did not cause hemolysis did affect the antigenic properties of human red blood cells.

Materials and Methods

The cancer high speed supernatants were prepared from human pulmonary carcinomas as previously described,[5] but high speed centrifugation

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is now being done for 120 min. at 24,500 g.

The red blood cells were obtained from patients with lung or breast carcinoma, occasionally from patients with carcinoma of the prostate or pancreas and sometimes from patients with other neoplasms, all of which were confirmed by microscopic examination. Most patients were tested before they had surgery, that is, those with relatively "early" lesions as far as could be determined and without clinical evidence of metastases or of anemia. With three exceptions, they had no blood transfusions or X-ray therapy. For controls blood was taken at the same time from healthy volunteers of the same ABO and Rh blood group and often also of the same MN type. The red blood cells were collected in Alsever solution under aseptic conditions, washed 4 times with cold saline, packed, and used within 6 to 8 hours after withdrawal.

Procedure

The entire two-step procedure was performed under aseptic conditions.

First step. Two ml. of cancer high speed supernatant and, as control, 2.0 ml. of saline or normal human serum diluted 1:10 or 1:15 were mixed with 0.1 ml. of packed red blood cells. The mixtures were incubated in a 37° C. waterbath for 120 min. and kept overnight at 4° C.

Second Step. The supernatants were removed and the red blood cell sediments were thoroughly washed three times in sterile cold saline and were employed for a 2 per cent red blood cell suspension. One-tenth ml. of this red blood cell suspension was added to 0.2 ml. of cancer high speed supernatant, to 0.2 ml. of saline, or to 0.2 ml. of normal human serum, undiluted or diluted 1:5. All materials corresponded with regard to the blood group. The supernatant-, saline-, or serum-red blood cell mixtures were incubated for 120 min. in a 37° C. waterbath, and hemolysis was recorded immediately after incubation and on the next 2 days after being kept at 4° C.

The degree of hemolysis, ranging from 1+ to 4+, was estimated from the color of the supernatant and the size of the sediment. In addition, in several instances the optical densities of the supernatant fluids were measured in the Coleman Junior spectrophotometer at a wave length of 550 mμ.

Results

During the *first step of the procedure* – incubation of the red blood cells with the supernatants – hemolysis occurred only rarely. Of 13 recently employed lung tumors, only 5 provided high speed supernatants that had a direct hemolytic effect on human red blood cells, and only 2 were strongly hemolytic. This confirmed our previous observations. Supernatants prepared at different times from the same tumor yielded the same results as regards the presence or absence of hemolytic property, but the hemolytic factor was lost if the tumor was stored at -20° C. for a con-

siderable length of time. Hemolysis, varying in degree from 1 to 3+, developed either immediately after incubation at 37° C. or on the next day after storage at 4° C. In all 10 experiments where there was a direct hemolytic effect of the supernatants, hemolysis of the red blood cells from cancer patients was persistently weaker than hemolysis of the red blood cells from healthy donors (1+ compared to 2 or 3+) or no hemolysis at all compared to 1+ of the normal red blood cells (FIGURE 1.) Optical density measurements of the supernatants showed the same pattern. The data of 3

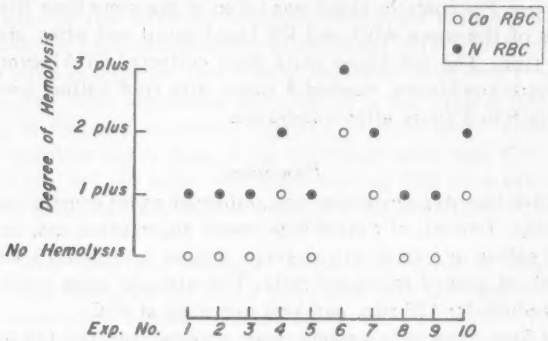

FIGURE 1. Difference in the hemolytic behavior of red blood cells from cancer patients and from healthy volunteers.

representative experiments are given in TABLE 1. A similar difference between red blood cells from cancer patients and those from healthy donors had been observed previously in 5 instances but was not reported because of the small number of tests. Now, however, these number 15. A difference was also found in titer. Red blood cells from noncancerous donors were hemolyzed by a strongly active supernatant in dilution 1:4, whereas the red blood cells from the cancer patients were hemolyzed by the supernatant only when undiluted.

TABLE 1

DIFFERENCE IN THE HEMOLYTIC BEHAVIOR OF RED BLOOD CELLS FROM CANCER PATIENTS AND FROM HEALTHY VOLUNTEERS

Optical density at 550 mμ. wave length

	Ca supernatant incubated with		Saline (control) incubated with		First wash fluid after incubation with Ca supernatant from	
Experiment No.	Ca RBC*	N RBC†	Ca RBC*	N RBC†	Ca RBC*	N RBC†
7	0.715	0.980	0.052	0.072	0.206	0.306
9	0.530	0.580	0.063	0.065	0.120	0.188
27	0.309	0.369	0.046	0.030		

*Ca RBC indicates red blood cells from cancer patients.
†N RBC indicates red blood cells from healthy donors.

In some instances there was no hemolysis but rather a clumping of the red blood cells that had been incubated with supernatants. Clumping may have occurred in earlier experiments but was recorded only in the later tests.

It should be added that control tests with red blood cells incubated with high speed supernatants from noncancerous lung were not satisfactory. The hemolytic effect of lung supernatants differed considerably from that of cancer supernatants and did not reveal a reproducible pattern. Some lung supernatants did hemolyze, others from the same lung did not. When hemolysis occurred, it developed rapidly and was sometimes complete during the first hour of incubation.

During the *second step of the procedure* the damaging effect of the cancer supernatants on the red blood cells was much more obvious than during the first step. It became evident that red blood cells that had been incubated with a cancer supernatant had acquired an increased tendency to hemolysis even if they looked normal on gross examination and under the microscope. When these pretreated red blood cells were mixed again with a cancer supernatant, not necessarily derived from the same tumor previously employed (but of the same blood group), hemolysis was much stronger than with the saline controls. This also occurred frequently in instances where hemolysis had not been noted after the first contact with the cancer supernatant. Supernatants of only four tumors did not hemolyze after the second incubation. Where hemolysis had occurred after the first contact with the supernatant, it was much stronger after the second. There was also a close association between the degree of hemolysis of the red blood cells and the strength of the supernatant—hemolyzing or not hemolyzing—employed for the first incubation. The more concentrated and the larger the amount of the supernatant, the stronger was the hemolysis after the second incubation (as seen in TABLE 2). Very minute amounts seemed to have an inhibiting effect on hemolysis.

Interestingly enough, during the second incubation, the effect of the cancer supernatants was about the same on red blood cells from cancer patients as on those from healthy donors. Weaker hemolysis of the red blood cells from cancer patients was found in only 8 of 30 experiments. In 22 experiments, the red blood cells from cancer patients and normal controls reacted either similarly or the hemolysis was stronger with erythrocytes from cancer patients. At this stage the initially observed resistance of the red blood cells from cancer patients was no longer present; it was often replaced by a tendency towards increased breakdown.

It was not always necessary that the red blood cells that had been in contact with a supernatant be submitted anew to the effect of a cancer supernatant. Reincubation with 0.85 per cent saline sometimes sufficed to produce a 1 or 2+ hemolysis. This occurred in 18 of 43 experiments. It was in sharp contrast to the reaction of red blood cells that had been pretreated with saline or with normal diluted serum. Such cells showed

TABLE 2

EFFECT OF VARIOUS AMOUNTS OF "NONHEMOLYZING" CANCER HIGH SPEED SUPERNATANT ON HEMOLYSIS OF RED BLOOD CELLS AFTER SECOND INCUBATION WITH SAME SUPERNATANT

Experiment No. 28	Amount of supernatant:	0.7 ml. 10%	1.3 ml. 10%	0.64 ml. 20%	1.3 ml. 20%	saline control
	Degree of hemolysis:	(+)	+	+	++	trace
Experiment No. 29	Amount of supernatant:	1.9 ml. 10%	0.8 ml. same, threefold concentrated by ultrafiltration and dialysis against saline.			
	Degree of hemolysis:	+	++			
	Opical density of supernatant at 550 mμ:	0.440	0.585			

weak hemolysis only twice, but usually failed to hemolyze. In 8 instances clumping was encountered immediately after reincubation in saline. This clumping, easily dispersed, temporary in nature, and barely seen on the next day, was also limited to red blood cells that had been pretreated with cancer supernatants.

Attempts to define the hemolytic properties of the cancer high speed supernatant more clearly have been greatly handicapped by its instability and by its sensitivity to temperatures above 5 to 10°C. Dialysis against saline for 3 days at 4° C. did not destroy hemolytic activity, indicating that substances of high molecular weight were involved.

We could not establish a relationship between the hemolytic effect of the supernatants and the presence or absence of necrosis in the individual tumor from which the supernatant was prepared, as was reported for saline suspensions from animal tumors.[6-8] "Nonhemolyzing" and hemolyzing supernatants were derived from tumors very much alike in their microscopic structure, showing more or less necrosis.

A study was also undertaken on the possible effect of nonhemolyzing cancer supernatants on the red blood cells. Rabbits were immunized with human 0 red blood cells that had been incubated with cancer supernatants or with saline. The antisera procured from these rabbits were absorbed with normal 0 red blood cells and were tested for agglutination of nonmodified and of trypsin-treated red blood cells. It was found that complete specific absorption with nonmodified red blood cells of the antisera to supernatant-treated cells was easily achieved by 3 or 4 absorptions, but a strong agglutinin for trypsin-treated red blood cells was still present

up to dilutions 1:8 and persisted after 3 more absorptions. By contrast, antisera to saline-treated red blood cells could rarely be absorbed specifically with nonmodified red blood cells. After 7 absorptions, in all but two experiments, weak agglutinins were still present for nonmodified and trysin-treated cells. In the two experiments mentioned, absorption was complete for both cells. This hemagglutination pattern is similar to that observed with antisera prepared in rabbits with red blood cells from cancer patients and from healthy donors (TABLE 3).[9]

TABLE 3

HEMAGGLUTINATION PATTERN AFTER ABSORPTION OF THE ANTISERA* WITH NONMODIFIED RED BLOOD CELLS

Anti-Ca sup-RBC serum		Anti-Sal-RBC serum		Anti-Ca RBC serum		Anti-N RBC serum	
Absorbed N RBC tested with				Absorbed N RBC tested with			
N RBC	Tryps. RBC	N RBC	Tryps. RBC	N RBC	Tryps. RBC	N RBC	Tryps. RBC
-	+	+	+	-	+	+	+
		or -	-			or -	-

N RBC indicates nonmodified red blood cells; Tryps. RBC indicates trypsin-treated red blood cells.

*See text.

Discussion

It has been shown that high-speed supernatants derived from human pulmonary carcinoma have a hemolytic and, occasionally, a clumping effect on human red blood cells. This effect may not be evident immediately. Hemolysis or clumping may develop only after a second contact with the cancer supernatant and after reincubation. It would seem that this delayed reaction is merely due to a cumulative effect of the cancer supernatant, the more so since there is a close association between the amount of supernatant used for the first contact and the degree of hemolysis developing after the second. There were some instances, however, where hemolysis of the red blood cells, pretreated with a cancer supernatant, developed on reincubation with saline only without further addition of a supernatant. Also there were other instances in which supernatants were employed that did not bring about hemolysis at all, not even after the second incubation. In spite of that, these nonhemolyzing supernatants left their imprint on the red blood cells by changing their antigenic properties. The nature of this change is still under investigation.

What relationship these observations have to those of Ludogovskaya and Shershulskaya as cited by Zilber[10] is open to question. These authors found by means of anaplylaxis and specific desensitization that "antigen from human tumors is adsorbed on the red blood cells in a considerable percentage of cases." It would seem that in our experiments a process is involved that is more intricate than adsorption.

Summary

High speed supernatants prepared from human pulmonary carcinoma act on human red blood cells in various ways. They may hemolyze the red blood cells on first contact or only after pretreatment and second incubation, or they may, without any evidence of hemolysis, affect their antigenic characteristics.

Red blood cells from cancer patients showed an initial resistance to hemolysis and were hemolyzed to a lesser degree than red blood cells from healthy volunteers. This initial resistance was followed by a tendency to hemolysis equal to or even greater than that of normal red blood cells.

Acknowledgments

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DIVISION OF ANTHROPOLOGY

ROCKS THAT RING: THEIR RITUAL SETTING IN NORTHERN NIGERIA*

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What is a rock gong? In the sense employed here, it is any rock showing signs of regular use in producing one or more musical notes. Rocks that ring are common the world over, but ringing rocks used as percussion instruments, that is, as rock gongs, are something of a rarity. To date, they are known to occur only in Europe and Africa. The literature on the subject implies that rock gongs are part and parcel of a megalithic culture complex to be found in prehistoric Europe, in Africa and, possibly elsewhere. If this is the case, then rock gongs and the traits said to be associated with them are of great importance in the study of man's prehistory.

In this paper I hope to contribute something to our knowledge of the use of rock gongs in Africa, since it is by extrapolating from contemporary customs that writers on the topic invest rock gongs with much of their significance for prehistory. I also shall take this opportunity to make what lawyers call a "fair commentary" on the implications being made in the literature on rock gongs and, finally, I have a suggestion of my own for the interpretation of these remarkable percussion instruments. Initially, however, it may be helpful to give a general description of rock gongs and briefly to review their distribution in Africa and Europe.

Rock Gongs and Their Distribution

The existence of rock gongs in Africa was unsuspected until only five years ago, when Bernard Fagg of the Nigerian Antiquities Service made his first discovery of a gong in Northern Nigeria (Fagg, 1956). Since 1955

*This paper, illustrated with film and tape recorder, was presented at a meeting of the Division on November 28, 1960.

The Division of Psychology held a meeting on November 21, 1960, at which Eugene S. Gollin of the Department of Psychology, Queens College, Flushing, N.Y. presented a paper on "Developmental Investigations of Cognition." This paper will be published in a subsequent issue of the *Transactions*.

The Division of Oceanography and Meteorology held a meeting on November 22, 1960, at which Frank W. Burnett of the National Weather Analysis Center, United States Weather Bureau, Suitland, Md., presented a paper on "Automatic Trends in Forecasting Weather: Utilizing Electric Computers." This paper will be published in a subsequent issue of the *Transactions*.

The Division of Biochemistry held a meeting on November 22, 1960, at which Frank F. Davis of the Department of Agricultural Biochemistry, Rutgers, The State University, New Brunswick, N.J., presented a paper on "Minor Components in Ribonucleic Acids." This paper will be published in a subsequent issue of the *Transactions*.

at least 30 gongs have been reported in Africa. These gongs are alike in not having been modified in any way – by chipping, grinding, or perforation for suspension – in order to control the quality or quantity of emitted sound.

As percussion instruments, rock gongs contrast with lithophones, stones modified by various techniques in order to yield a controlled range of notes (Fagg, 1956, page 17). Lithophones are believed to occur in Africa only in association with the Coptic Church of Ethiopia (Bennet-Clark, 1957, page 32), but are widely distributed elsewhere, including the Americas (see, for example, Brugge, 1956). At least on the basis of our present knowledge, unmodified rock gongs are limited to Europe and Africa. It must be emphasized, however, that rock gongs are easily overlooked and, eventually, they may be reported from other parts of the world.

In Africa gongs often are found in the debris of younger granite outcroppings (Fagg, 1957, page 306). Among many hundreds of boulders, large and small, making up this debris, the only distinguishing feature of the gong is the presence of "chatter marks" – small, cuplike depressions – caused by repeated, nonrandom striking of the rock with a hammerstone, often of quartz or quartzite. These chatter marks appear along the perimeter of all ringing rocks that are, or have been, in regular use as gongs. The sizes of gongs vary greatly. One may be as large as a locomotive, while another is less than a cubic foot in volume. The quality of the note apparently is determined by the volume of rock that is free to resonate rather than by over-all size or weight.

In playing a gong, the hammerstone – being relatively fragile – quickly shows signs of wear. An expert player rotates the hammerstone so that all its surfaces are smoothed off, and the hammerstone itself soon becomes spherical. One or both hands may be used, and as many people may play a gong as can crowd around the striking area. Also, more than one gong may be played at a time.

Gongs thus far reported from Africa lie within a broad zone south of the Sahara and north of the Equator. Of about 30 sites definitely described in the literature, approximately 24 are in Nigeria, all but two of which are in the Northern Provinces. The two exceptions include one site in the Northern Cameroons (near Gwoza) and another – claimed to be the most southerly in West Africa – at Igbetti, near Old Oyo, in the Western Provinces of Nigeria (Morton-Williams, 1957, page 170). The large number of gongs in Nigeria as compared to elsewhere in Africa may be due less to the possibility that Nigeria is an epicenter of a prehistoric gong culture than to the enthusiasm with which Fagg has fired Nigerians and Europeans, myself included, in searching out and reporting gong locations. East of Nigeria, two gong sites are known in Uganda (Lanning, 1959, pages 84-5); at Shoba, about 100 miles west of El Fasher in Darfour (Davidson, 1959, page 85); and, possibly on the west bank of the Nile at the Sixth cataract (Fagg, 1956, page 19). A rock slide, to be discussed later has been

found near Lusaka in Northern Rhodesia (Fagg, 1957*a*, page 31).

Unmodified rock gongs have been reported outside of Africa from Portugal, Brittany, Wales, and England (respectively, Davies, 1959, page 86; Ettlinger, 1956, page 75, 1959, pages 85-6; Fagg, 1957*a*, pages 31-2, 1957*b*, page 112; and Williams, 1957, page 188). Fagg believes that some of the Brittany gongs are similar in size, shape, and tonal quality with those of Nigeria (Fagg, 1957*a*; page 31).

Given their variety in size and shape and the inconspicuousness of the chatter marks (overlain sometimes with lichen and considerably weathered), the reason why gongs went so long unnoticed in Africa is apparent. Without Fagg's original discovery, it is entirely possible that I should have remained ignorant of about 14 gongs in the area where my own fieldwork was carried out in Northern Nigeria.* This is true not only because gongs are difficult to recognize, as noted above, but also because their use in ritual is surrounded with great secrecy.

The Use of Gongs among the Barawa and Bankalawa

The Bankalawa and Barawa people inhabit the plains and mountains, respectively, of Dass Independent District, Bauchi Province, Northern Nigeria. While less than 300 square miles in area, the district includes the foothills on the southeastern slope of the Jos-Bauchi Plateau as well as the plains or flatlands that extend nearly uninterruptedly to the Benue River Valley, some 200 miles to the south. The Barawa mountaineers speak a Chadic language and the Bankalawa a dialect belonging to the Central Branch of the Niger-Congo family of languages (Greenberg, 1955, page 116). In Dass, both the Barawa and the Bankalawa have remained pagan, although the latter (by virtue of their plains habitat and proximity to trade routes and large markets) have been subject to strong acculturating forces from Muslim and Muslim-oriented sources.

In Dass, among the Bankalawa and Barawa, rock gongs are used in two different contexts: as warning devices and as paraphernalia associated with the ritual of initiation. Rocks that simply ring are scattered throughout the hills of Dass and are the occasional playthings of goat boys and other indigents. Very few of these rocks show any indication of more than desultory use.

At nine different sites, gongs were found employed as warning or signaling devices. Seven of these sites (Old Wandi, Old Gyamus, Kuletu, Dot, nJalom, 'Dir, and Old Galagam) are abandoned villages on the higher slopes of the Dass Hills. The gongs are located precisely where a sentry or lookout could have kept under observation most approaches to the village and the plains below them. This makes it seem likely that the gongs were carried or rolled into position. None were of such size as to make their transport impossible. Two warning gongs were discovered on the Dass

*As a Fellow of The Ford Foundation, 1957-1959.

plains: one of them at "Bununu I," the first site occupied by Jarawa immigrants in Dass; the other on the outskirts of the Bankalawa community Bundot.

It is in the context of pagan ritual that the use of gongs becomes a matter of great secrecy. Like all ritual equipment in Dass, gongs are owned or controlled by individual cult leaders who are in competition with each other for clientele and followers (Conant, 1960). The lure of the unknown, the power of suggestion, hints, and rumors, are all psychological weapons of the cult leaders, and the air of mystery they encourage pertains as much to rock gongs as to any other item of ritual equipment.

The ritual context in which gongs are used in Dass apparently is limited mainly to initiation that takes place every four years among the Barawa, and five or seven years among the Bankalawa. Cult leaders set aside their rivalries to cooperate – albeit sometimes unwillingly – for the purpose of staging the initiation ritual, which includes a period of isolation of the initiates lasting from one week to one month. Presistent questioning of informants at the Bankalawa village of Lir yielded the information that the set of gongs used in their initiation had been given them "in the time of their ancestors" by the chief of Gwolthir, at present a Bankalawa-Jarawa village about 20 miles to the south of Lir. Bankalawa informants at Galagam also admitted an outside source for the set of gongs employed at their own initiation site, but refused to reveal the donors. A variety of evidence leads me to suspect that this source was the Barawa mountain settlement of either Zumbul or Old Polchi. The entrustment, exchange, and even deliberate display of ritual equipment, which otherwise is cloaked in secrecy, is a regular technique among the peoples of Dass for the maintenance of inter-community relations (Conant, 1960*a*).

At Lir gongs are played ensemble, five at a time, two players at each gong. The largest gong of the set is about 3½ feet long, of an irregular lozenge shape, about 1 foot thick, and 1½ feet across at its widest. This gong, which was spoken of as the "father" of the four others, was also the largest and was the only one informants agreed to play for my benefit. At Galagam two small gongs were played. Each was triangular-shaped, the greatest length being about 1½ feet and less than 6 inches thick.

Rock gongs in Dass are regularly spoken of as *gigyun*, apparently a borrowing and subsequent modification of the Hausa word *kuge*, used in reference to the double-iron hand gong having a wide distribution in Africa. In Dass, iron gongs are of some antiquity and are still in use among the Bankalawa and Jarawa pagan plainsmen as emblems of chiefly office. The iron gongs are beaten at times of crisis in order to summon advisers to the chief's residence. Musicians in Dass also use the iron gong as one of a number of accompanying instruments in singing the praises of a Muslim chief. That some of the rock gongs may have been adopted by the mountaineers in imitation of the double iron gong appears to be a likely possibility that does not detract from the way in which rock gongs "fit"

the general context of the initiation ritual among the mountaineers.

Characteristic of this ritual, and indeed of the entire pagan religion of the peoples of Dass, is a consistently great emphasis on audible as well as visual representations of the supernatural. That is, any visual impersonations of supernatural beings always have their audible counterparts. For example, a generalized ancestor figure, referred to loosely by the Hausa term *dodo*, is impersonated by costumed dancers who also speak and sing in a special ritual language with grammatical, lexical, and phonetic features all of its own.

Where rock gongs are not in use, songs sung in the "voice of *dodo*" are accompanied by a type of drum in wide use among Plateau pagans. Drums and rock gongs, however, are never played together in Dass. On the basis of a preliminary comparison, it appears that there are no special rhythms or beats particular either to drums or to gongs (Colin Turnbull, American Museum of Natural History; New York, N.Y., personal communication). Similarly, there is little if any significant variation in the kinds of songs sung to the accompaniment of either drums or gongs. In Dass, in other words, rock gongs may replace drums and vice-versa without apparently altering the context of the initiation ritual itself. It is against this background that I shall consider some of the interpretations that have been made in the literature on rock gongs and, finally, offer my own interpretation.

The Rock Gong "Complex"

Although the literature on rock gongs is scarcely five years old, a convention already has arisen to write of a rock gong "complex" (Morton-Williams, 1957, page 170). The elements making up this complex are said to be three: rock gongs themselves, rock paintings, and rock slides. These last are more or less steeply inclined rock surfaces down which one may slide using a variety of devices, including stone, as a sled. The impression has been created that if one of the foregoing elements is found, a more thorough search may turn up the other two. In an early article, Fagg associates the rock gongs with paintings discovered at Birnin Kudu, Kano Emirate because of the fact that, whereas ringing rocks – which show no signs of having been used as gongs – are common throughout the area, "the rock gongs themselves are usually clustered within about 100 feet of the paintings" (Fagg, 1956, page 17). Fagg also notes that "the brides of Birnin Kudu (now a staunchly Mohammedan community) go to this cave at dawn on their wedding day and return in the late afternoon to the town for their wedding ceremony. As they are apparently oblivious of the paintings it seems likely that this custom is a vestigial survival of rites, now forgotten, that were connected with the paintings and rock gongs a few feet away" (Fagg, 1957, page 17). In a later article, Fagg raises the interesting possibility that "the exceptional sanctity of the Prescelly Mountains" in Wales, which he cites as the source of the Blue Stones of

Stonehenge, was due "in some measure to the ringing quality of so many of its rocks" (Fagg, 1957*a*, page 31). The presence of rock slides in Wales is also noted, as well as the conjunction of rock gongs and slides in Brittany. The article concludes with the statement that "intensive study" of the "occurence and association" of rock gongs and slides with paintings and petroglyphs "may significantly assist in the interpretation of prehistoric cave art" (Fagg, 1957*a*, page 32).

At this point, I think a "fair commentary" is in order. Although the rock gongs of Birnin Kudu are described as being in an "archaeological context" with the rock paintings at the same site (Fagg, 1956, page 17), the case, it seems to me, is far from proved. To prove it, it would appear necessary to find hammerstones amid the cultural debris already dug up from the floor of the cave in which the paintings are found. No such hammerstones are listed among the objects recovered in excavating the site (Fagg, 1957, page 309). The "intensive study" of rock gongs that Fagg has called for (1957*a*, page 32) should be completed at Birnin Kudu, or elsewhere in Africa, before being extended to Western Europe. Otherwise the assumed association between gongs, slides, and paintings rests on little more than: (1) their occasional conjunction at the same site or (2) survivals of "vestigial rites" and extrapolation from the contemporary use of gongs and slides in Africa. I shall comment on these two points as follows:

(1) To the best of my knowledge, rock slides are not associated in any discoverable way in Dass with rock gongs although, admittedly, rocks that ring may be found in the same general area as rocks that slide. No rock paintings have been found in Dass despite my best efforts at their discovery, and this important part of the "complex" apparently is lacking. If the simple conjunction of gong, slide, and painting is to be the basis for assuming some inherent nexus among them, it would seem necessary to demonstrate that each element is associated with the other two more frequently than chance alone would permit. No one thus far has shown that such a greater-than-chance correlation exists. A sufficient explanation for their occasional conjunction would be that gongs, slides, and paintings all employ a common material: rock.

(2) There is little in the ethnographic data from Dass that supports more than a tenuous association of rock gongs and fertility rites. It is true that some of the songs sung at the initiation of young boys pertain to fertility and general increase. For example, a common refrain is "the penis of Wandi shall eat the vagina of Bununu." This song, however, is sung to the accompaniment of drums. A similar song is sung, but with the names of the towns changed, at Lir, where rock gongs are used at the initiation ritual. It would thus appear that the rock gong per se stands in no special relationship to concepts of fertility: in view of the pervasive West African concern with fertility and general increase, it would be most surprising if gongs were to do so. With respect to rock slides, while young

girls in Dass are remarkably free to engage in other activities, sliding down rocks is not one of them. Fagg has noted correctly that rock gongs and slides may be found near the slabs of rock on which girls practice grinding crops (Fagg, 1956, page 19). If this is the basis for associating rock gongs with fertility concepts, then it seems to me that such an association could have been arrived at independently many times and at many different places in Africa, in Europe, and elsewhere in the world wherever rocks are used for grinding or pounding crops. For example, on the Micronesian islands of Ponape and Kusaie, basalt rocks on which *kava* is pounded are selected specially for their tonal quality and are played ensemble by three to five men using "large pebbles" as hammerstones (S. H. Riesenberg, Curator of Ethnology, Smithsonian Institution, Washington, D.C., personal communication). Need such a practice be interpreted as the vestigial survival of a custom inherited from prehistoric times? Apparently not, since Riesenberg informs me further that one of the rhythms used in playing the gongs on Ponape was described as being "in imitation of the sound made by coopers aboard whaling ships 100 years ago" (personal communication).

The ethnographic data from Dass also show that for some groups, notably the Bankalawa of Lir, possession of a gong may be a comparatively recent acquisition from an external source (see above). The manipulation of ritual equipment, including gongs, for political purposes in Dass makes it clear that contemporary possession of a trait is no certain proof of a prehistoric inheritance. This seems further borne out by the way in which gongs and drums displace each other in the performance of initiation with little or no correlative changes in the nature of the ritual itself.

An Alternative Interpretation of Rock Gongs

Fieldwork in Dass prompts the following question: may the use of rock gongs represent a substitution of abundantly available ringing rock for the double hand gong made of iron, a much more scarce material? The quality of notes produced by both instruments is so similar that it is sometimes difficult to tell them apart. The iron gong has a wide distribution throughout the Sudan and Africa including the Rhodesias (Walton, 1955, 1957). Fagg has reported a rock slide in Northern Rodesia (see above, page 3). Will ringing rocks be found there as well as sliding rocks? If so, will these rocks show the characteristic "chatter marks" of gongs? Again, if so, what is the local history and use of the double iron hand gong? In other words, it would be most suggestive if rock gongs and iron gongs eventually prove to have roughly the same distribution. The significance of rock gongs then might be largely in terms of the diffusion of iron metallurgy in Africa, associated by some prehistorians with the spread of Bantu-speaking peoples (Clark, 1959, Pages 281-313; Cline, 1937, pages 141-142; Mauny, 1952, pages 545-595). Such an interpretation is admittedly

more limited than that which sees rock gongs as part of a prehistoric, megalithic complex. However, this does not detract from the importance of Fagg's original discovery, nor does it lessen our indebtedness to him for so vigorously pursuing what I hope to have shown to be the complex subject of rock gongs, rather than a rock gong complex.

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CONFERENCES HELD

SECTION OF BIOLOGICAL AND MEDICAL SCIENCES

GENETIC PERSPECTIVES IN DISEASE RESISTANCE AND SUSCEPTIBILITY

Thursday, November 17, and
Friday, November 18, 1960

Honorary Conference Chairman: The Hon. Henry A. Wallace
Farvue Farm, South Salem, N. Y.

Organizing Chairman: Richard H. Osborne,
Sloan-Kettering Institute for Cancer Research, New York, N. Y.

THURSDAY, NOVEMBER 17, 1960

9:00 A.M.–
Greetings from the Academy - Charles Noback, Vice-Chairman, Section of Biological and Medical Sciences, The New York Academy of Sciences, and the College of Physicians and Surgeons, Columbia University, New York, N. Y.

Introductory Remarks - The Hon. Henry A. Wallace, Farvue Farm, South Salem, N. Y.

ADAPTATION IN MAN AND ANIMALS

Session Chairman: Theodosius Dobzhansky
Columbia University, New York, N. Y.

"The Host Factor in Disease: Heredity-Environment Interaction" - Richard H. Osborne, Sloan-Kettering Institute for Cancer Research, New York, N. Y.

"Problems in the Environmental Adaptation of Domestic Animals" - Douglas H. K. Lee, Occupational Health Field Headquarters, United States Public Health Service, Cincinnati, Ohio.

"Biological Adaptation of Man to his Environment: Heat, Cold, Altitude, and Nutrition" - Marshall T. Newman, United States National Museum, Washington, D.C.

Synthesis: Theodosius Dobzhansky, Columbia University, New York, N. Y.

GENETIC ERRORS OF METABOLISM AND ENVIRONMENTAL INTERACTION

Session Chairman: David Yi-Yung Hsia
Children's Memorial Hospital, Chicago, Ill.

2:00 P.M.–

"Heredity Disorders of Acid Mucopolysaccharide Metabolism in Humans and in Snorter Dwarf Cattle" - Andrew Lorincz, University of Florida Medical School, Gainesville, Fla.

"Genetic Variation in the Utilization of Riboflavin, Thiamine, and Other Nutrients" - Frederick B. Hutt, New York State College of Agriculture, Comell University, Ithaca, N. Y.

"Familial Hypophosphatemia: An Inherited Demand for Increased Vitamin D" - John B. Graham, University of North Carolina School of Medicine, Chapel Hill, N. C.

Synthesis: David Yi-Yung Hsia, Children's Memorial Hospital, Chicago, Ill.

FRIDAY, NOVEMBER 18, 1960

GENETIC RESISTANCE AND SUSCEPTIBILITY TO INFECTIOUS DISEASE

Session Chairman: Howard Schneider
The Rockefeller Institute, New York, N. Y.

9:00 A.M.–

"Experimental Analysis of Genetic Determinants in Resistance to Infectious Disease" - John W. Gowen, Iowa State University, Ames, Iowa.

"Genetic Factors Affecting Resistance to Malaria" - Anthony C. Allison, National Institute for Medical Research, London, England.

"Finding Genetic Determinants of Communicable Disease in Human Populations" - Morton D. Schweitzer, School of Public Health and Administrative Medicine, Columbia University, New York, N. Y.

Synthesis: Howard Schneider, The Rockefeller Institute, New York, N. Y.

GENETIC METHODS FOR EPIDEMIOLOGICAL INVESTIGATIONS

Session Chairman: Ching Chun Li
Graduate School of Public Health
University of Pittsburgh, Pittsburgh, Pa.

2:00 P.M.–

"The Genetic Interpretation of Family Studies of Disease" - Lyman B.

Crittenden, Experimental Statistics Section, Biometry Branch, National Cancer Institute, Public Health Service, Bethesda, Md.

"Statistical Studies of Probands and Their Relatives" - V. Elving Anderson, Dight Institute for Human Genetics, University of Minnesota, Minneapolis, Minn.

"Problems and Areas in Genetic-Epidemiologic Field Studies" - Abraham B. Lilienfeld, School of Hygiene and Public Health, Johns Hopkins University, Baltimore, Md.

Synthesis: Ching Chun Li, Graduate School of Public Health, University of Pittsburgh, Pittsburgh, Pa.

SECTION OF BIOLOGICAL AND MEDICAL SCIENCES

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Elected November 17, 1960

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